

All things considered, then, it appears probable to me that with the spread of birth-control information moderate reductions in the populations of many countries will be recorded at no very distant date. But far from regarding these reductions as catastrophes or menaces to the well-being and happiness of the masses of mankind, I must say emphatically that I hail their advent as a blessing. The many reasons that have led me to adopt this attitude I have stated in detail in my recent book, *Birth Control and its Opponents*.

In conclusion, speaking as convinced eugenist and birth-controller rolled into one, I would earnestly plead with all eugenists not to allow statistical shadows of a distant future to obscure from their view the very real sufferings of the poor in many lands to-day, and I would urge them on no account to give their support to any policy, based on those shadows, which may antagonize their brethren of the birth-control movement. For any real disunion among workers in the field of population—that would be catastrophe indeed.

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View on Race and Eugenics: Propaganda or Science?

To the Editor, Eugenics Review

SIR,—If Professor Gates had written that infertility as the *only* criterion of species is out of date, I should quite agree with him. On the other hand, in all higher animals, so far as I am aware, even when crosses between species prove to be wholly infertile, the two groups, if adjudged good species by the systematist on other grounds, do not in point of actual fact cross with each other on a large scale. The intercrossing is either a sporadic aberration, or occurs only along the boundary of the ranges of the two groups.

This is precisely what does *not* occur in man. By migration, the different groups of human beings have been brought into contact with each other on a large scale and intercrossing does freely occur over large portions of the range of the group. Thus even if the four main types of man mentioned by Professor Gates had evolved in such entire isolation as he asserts (which is by no means certain, since the isolation may not have been complete), and even if they had at one time merited the name of species (which again is a matter of systematic taste: many would regard them as sub-species), the human group to-day is in point of fact a freely-intercrossing one, and any attempt to label it as consisting of a number of separate species would, in my opinion and that of numerous other biologists, lead to an impossible situation for systematics. In any case, the main point at issue is not the precise terms to be employed, but whether the situation in man is or is not in numerous essential respects different from that in other mammals.

Professor Gates once more asserts that Dr. Haddon and I have been guilty of propaganda, on the ground that the book "seeks to deny that even human races exist." What we have actually asserted is that, although groups meriting the term *race* appear certainly to have existed in the past, they do not do so to-day (save possibly in a few small and out-of-the-way communities) and that the term has connotations which make its use inimical to further scientific progress. In this we were concerned solely with scientific terminology and methodology. In any case, we are by no means alone in our views, as was made evident by the joint discussion on the subject at the British Association this summer. I accordingly see no reason for withdrawing my protest against what I regard as a wholly unjustified accusation.

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To the Editor, Eugenics Review

SIR,—Professor Gates writing in your last issue in support of his views on the nature of human races, makes the truly astonishing statement that the species in "the group of the Bovidae including cattle, zebu, bison, yaks, etc.," are interfertile with only slight indications of sterility in certain crosses. Kuhn, Boyd and Iwanow respectively crossed cows (*Bos taurus*) with the yak (*Bos grunniens*) American bison (*Bison americanus*) and the European bison (*Bison bonasus*) as male. In each case they found that, although the female hybrids were fertile, the males were completely sterile. Unless Professor Gates has access to more recent data which contradict those cited above, his letter, to use his own phraseology concerning Dr. Huxley, "ranks itself as a propagandist rather than a scientific work."

An equally surprising statement in the same number is Dr. Cattell's on p. 19 that "about 75 per cent. of the children of the feeble-minded are also feeble-minded, and the remainder are not far above the border-line." On p. 183 of Vol. 25 of the *EUGENICS REVIEW*, out of 345 children of mental defectives in Birmingham only 7.2 per cent. are said to have been defective and another 18 per cent. backward. Seventy-five per cent. were of average intelligence or even above the average. Most investigators find a rather higher proportion of defectives, but frequencies of over 50 per cent. are quite exceptional.

If statements of this kind are used to support the eugenics movement a certain number of scientific students of heredity are likely to hold aloof from it. The *EUGENICS REVIEW* could do an immense service to eugenics by setting a higher standard of accuracy. Is it too much to hope that the opportunity will be taken?

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. The statement to which Professor Haldane takes objection in the second paragraph of his letter was made in the course of an article for which the author alone is responsible. While every care is taken in the selection of articles for publication it is considered neither possible nor desirable to exclude from the columns of the REVIEW controversial matters or expressions of personal opinion. The fact that we do not in all matters connected with eugenics find ourselves in complete agreement with Professor Haldane would not be a sufficient reason for rejecting any article he sent us on the subject. The views of the *Society* are expressed in the editorial columns, particularly in the Notes of the Quarter.

To the Editor, Eugenics Review

SIR,—Professor J. B. S. Haldane has rushed to the defence of Dr. Huxley on a point regarding interspecific sterility. He makes no attempt to controvert my main point, namely, that intersterility is no longer an essential criterion of species. He cites certain recent work in which he claims that in crosses of cows with yaks and bison, although the females were fertile the males were completely sterile. My statement was based upon the work of Lus (quoted in *Heredity in Man*, pp. 302-3), who found that in crosses between yaks and Kirghizian cattle "the F_1 female hybrids are fully fertile when crossed back with either parent species," while "the male F_1 hybrids are at least partially fertile." Zavodovsky made similar crosses between yaks and zebu.

Professor Haldane is entitled to any comfort he can derive from the difference in the two statements. They do not in any case affect the main thesis, which he makes no attempt to dispute. Even if the statement proves to be true that the males in these crosses are wholly and not partially sterile, it does not affect my statement that *We Europeans*, which contains whole chapters of tendentious statements, is a propagandist rather than a scientific work.

R. RUGGLES GATES.

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To the Editor, Eugenics Review

SIR,—The passage in my article "Is National Intelligence Declining?" to which Professor Haldane objects was obviously intended as one of a number of general introductory approaches to the subject of intelligence inheritance for the non-technical reader: the precise study of the problem follows in the extensive appendix. In any case, no psychologist, or, indeed, logician, would look for precision in a statement regarding a concept which is by its nature incapable of being used precisely.

Mental deficiency is not a concept of scientific psychology: it is a rough sociological and administrative notion for the use of the general practitioner. In what follows I propose to discuss the

possibilities of introducing accuracy into statements in this field.

Roughly speaking, mental deficiency is a product of two major deficiencies: (1) of "g"—manifested by an I.Q. below 70 or 75, (2) of emotional stability. Many persons with an I.Q. of below 50 go uncertified because they possess high emotional stability. Others as high as 80 will be found certified owing to emotional instability, sometimes under the psychologically obsolete label of "moral imbecility." Even on the purely intellectual side the standards and methods of ascertainment vary from time to time, from country to country, and, indeed, from town to town.

It is obviously misdirected science to expect to obtain any precise rule of inheritance with regard to such a conglomerate of powers. For example, since the instability for which a person of I.Q. 80 is mainly certified is often an acquired emotional maladjustment, the children of such a person, possessing the parental I.Q. but not having caught by contagion the emotional maladjustment, will rightly not be considered defectives—to the confusion of statistics.

Moreover, owing to the sterilization controversy, most enquiries have looked mainly for data regarding the percentages of defectives among the parents of defectives. That the figures obtained vary from 10 per cent. deduced by Dr. Blacker from the Brock results to the 60 per cent. asserted by Professor Berry is not surprising in view of the above confusions, but since all busy medical officers agree that the ascertainment was very chancy in the last generation, the figures obtained from administrative records will in general be distinctly too low. (Also through changes in name through marriage, etc.) Finally, owing to scatter and regression, the percentages of defective parents of defectives will be no guide to the percentages of defective children of defectives. The distribution curve shows far more people between I.Q.s 70 and 75 than below 70, consequently if we continue to refer to I.Q.s 70 to 75 as "normal intelligence" a considerable proportion of defectives will always be born of "normals."

The above considerations suggest that it would be best to make a direct approach by beginning with defective parents and studying their children. Because of relatively good ascertainment and standard conditions Professor Fisher's L.C.C. data are the nearest approach to being satisfactory. Unfortunately the results deal not with the children of defectives, but with the children of defectives and unspecified "normals." Of the offspring 45 per cent. were defective when the father was defective and 39 per cent. when the mother was defective. These figures must be lower than the true ones because: (1) Even to-day the ascertainment of defective children is far from perfect, partly owing to the teacher's reluctance to refer cases, especially those of quiet disposition and regular behaviour; (2) the L.C.C. data included only children over 6